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tion is quite indefensible as a permanent social condition. Nevertheless, capital, distinguished as a provision for offspring, is a eugenic institution; and unless human instinct undergoes some profound and improbable variation, abolition of capital means the abolition of effort; but as in the body the power of independent growth of the parts is limited and subordinated to the whole, similarly in the community we may limit the powers of capital, preserving so much inequality of privilege as corresponds with physiological fact.

At every turn the student of political science is confronted with problems that demand biological knowledge for their solution. Most obviously is this true in regard to education, the criminal law, and all those numerous branches of policy and administration which are directly concerned with the physiological capacities of mankind. Assumptions as to what can be done and what can not be done to modify individuals and races have continually to be made, and the basis of fact on which such decisions are founded can be drawn only from biological study.

A knowledge of the facts of nature is not yet deemed an essential part of the mental equipment of politicians; but as the priest, who began in other ages as medicine-man, has been obliged to abandon the medical parts of his practise, so will the future behold the schoolmaster, the magistrate, the lawyer, and ultimately the statesman, compelled to share with the naturalist those functions which are concerned with the physiology of race.

WILLIAM BATESON

THE STATUS OF HYPOTHESES OF POLAR WANDERINGS

FOR the past century, hypotheses which postulate a wandering of the earth's axis of rotation within its body have been advocated by various geologists and biologists as an explana-

tion of past climatic and biotic changes. Astronomers, on the contrary, have in general been opposed to hypotheses of polar migration; for in their opinion, not only is there no astronomic evidence pointing toward such instability of axis, but extensive and progressive wanderings are regarded as mechanically impossible. Geologists and biologists may array facts which suggest such hypotheses, but the testing of their possibility is really a problem of mathematics, as much as are the movements of precession, and orbital perturbations. Notwithstanding this, a number of hypotheses concerning polar migration have been ingeniously elaborated and widely promulgated without their authors submitting them to these final tests, or in most cases even perceiving that an accordance with the known laws of mechanics was necessary. Others, of more logical mind, recognizing the need of mathematical justification, have thought to find a qualified support in the work of Kelvin and G. H. Darwin. The chief point of this paper lies in showing that the work of Darwin, instead of permitting hypotheses of polar wanderings, offers the most convincing proof which is available that migrations of the axis of the earth sufficiently extensive to be of geological importance have not occurred. Darwin, in his conclusion, granted the possibility that the pole may have worked its way in a devious course some 10° or 15° away from the geographic position which it held *at the consolidation of the earth*, and he states that it may as a maximum have been deflected from 1° to 3° in *any one geological period*. This extreme limit to migration was purposely based upon those assumptions which might be geologically possible and which would permit the greatest changes in the axis of rotation. A reexamination of those assumptions in the light of forty added years of geologic progress suggests that the actual changes have been much less and are more likely to be limited to a fraction of the maximum limits set by Darwin. His paper seems to have checked further speculation upon this subject in England, but, apparently unaware of its strictures, a number of continental geologists and biologists have car-

ried forward these ideas of polar wandering to the present day. The hypotheses have grown, each creator selecting facts and building up from his particular assortment a fanciful hypothesis of polar migration unrestrained even by the devious paths worked out by others.

If these varied and contradictory hypotheses were kept merely as exhibits of those strange creations of the mind which are stored in the museum of pseudo-science, there would be little present need for a discussion of the subject; but such is not the case. Able workers in the fields of natural science, a number of them deservedly of the first rank, overlooking the fatal mathematical objections, impressed by the apparent authority of the originators of some of the hypotheses, and assuming that these authors had made a thorough investigation, have, while treating the subject cautiously, still given it serious attention. This is especially true of that very elaborated scheme of a pendulating earth put forth by Reibisch and voluminously supported by Simroth. It has been brought to the attention of the American scientific public in a favorable review by R. E. Richardson in *SCIENCE*,¹ and more lately by Grabau, who discusses this and other hypotheses of polar migrations on pages 891-899 of his recent work on "The Principles of Stratigraphy." In this work in fact the only hypotheses of climatic change through geologic time which receive detailed treatment are those of polar migrations, while certain important hypotheses, such as those of possible changes in the deep oceanic circulation, or changes in solar radiation, receive no mention. Although Grabau states that the pendulation theory is still too new and too little tested to receive more than respectful attention, he nevertheless regards it as a working hypothesis which is likely to be of much value, and from the space he devotes to it clearly considers it of much importance. The writer's high opinion of Grabau's "Principles of Stratigraphy" has been expressed recently in *SCIENCE* and it is because of his estimation of the importance of that work that this article

is written. The wide degree to which the "Principles of Stratigraphy" will be studied in America during the next decade will spread equally widely these ideas of polar migrations. There is need in consequence that the lines of counter arguments should be definitely set forth.

What then, briefly, are these hypotheses of polar wanderings and on what kind of evidence do they rest?

Some seventy years ago the proofs were developed of recent continental glaciation in middle latitudes. This was in striking contrast with the floral evidence of the mid-Tertiary warm temperate climate which prevailed in Greenland and Spitzbergen. The recognition of these great climatic changes in late geologic times gave rise to the suggestion that a migration of the poles seemed the simplest means of accounting for them. Sir John Lubbock in 1848 communicated a paper to the Geological Society of London upon this subject and Sir Henry De la Beche discussed it in his presidential address in 1849. Mr. John Evans in his presidential address to the same Geological Society in 1876 recurred to it. Evans, after describing a system of geological upheaval and subsidence, evidently designed to produce a maximum effect in shifting the polar axis, asks:

Would not such a modification of form bring the axis of figure about 15° or 20° south of the present, and on the meridian of Greenwich—that is to say, midway between Greenland and Spitzbergen, and would not, eventually, the axis of rotation correspond in position with the axis of figure?

It was in answer to these questions that George H. Darwin wrote his conclusive paper.

We may pass next to the far more extravagant demands of the hypotheses framed in later years and in disregard of Darwin's work. The exact references are all given by Grabau.

In 1901 Reibisch proposed a theory of polar pendulation, *i. e.*, a back and forth migration of the poles along a certain well-defined path. An axis of oscillation he supposed to pass through Ecuador and Sumatra. These points have consequently never changed in latitude. The axis of rotation is supposed to

¹ Vol. XXVIII., pp. 375-379, 1908.

remain always at right angles to this oscillation axis but to shift within the earth north and south along the meridian of 10° east of Greenwich. Thus the north pole is supposed in the Pleistocene to have lain north of Scandinavia and to be now advancing in the direction of Bering Sea. In the Jurassic and Cretaceous the continent of Europe is supposed to have been in tropic latitudes. An examination of the work of Reibisch shows no mention of the astronomic side of the problem nor any reference to the work of G. H. Darwin. His argument rests chiefly on various facts in the distribution of animals and plants and also upon the submergence and emergence of certain regions.

Kreichgauer in 1902 produced a map of the polar wanderings through geologic time as worked out by him, in which he shows the poles actually changing place, the north pole migrating from the Antarctic in the Pre-Cambrian northerly through the Pacific Ocean, through Alaska and the Arctic Archipelago to Greenland, and thence to its present position.

Jacobitti on the other hand prefers a different path, his north pole lying in the South Atlantic in Cambrian times, thence moving easterly across South Africa, India, Australia, the Pacific Ocean, Canada and Greenland to its present location.

Dr. Heinrich Simroth, professor in the University of Leipzig, elaborated the hypothesis of Reibisch, publishing in 1907 a book of 564 pages on "Die Pendulations Theorie."

These hypotheses rest chiefly upon facts and interpretations regarding the distribution of plants and animals. Support for them is also sought in the nature of crust movements and in the geologic evidences of past climatic changes. Much of the evidence is vague in delimitation and in significance, some of it is not clearly applicable, some of it could be offset by opposing evidence, and all of it can be given other interpretations which find a better geologic basis and do not contravene the laws of mechanics.

The writer has examined in some detail the hypothesis of Reibisch and Simroth, since this is the one which has been most commended

to geologists. Most of the following criticisms are directed toward their work, but in a general way they apply to the other hypotheses also. The kind of evidence upon which Reibisch founds a hypothesis involving a new earth motion of which he is the discoverer is seen in the following statements. He locates his oscillation poles in Equador and Sumatra because of botanical writings which claim that the Tertiary floras of those regions were not modified by Pleistocene climatic changes. Archaic and related types of animals inhabit these two antipodal regions, preserved from extinction because of the constancy of climate surrounding these oscillation poles. The oscillation circle is at 90° to these poles, running north and south through Europe and Africa. In the vicinity of this circle, climatic changes, owing to the poles moving back and forth on this meridian, are stated to have repeatedly driven out the faunas and floras and made this region that which has promoted the greatest evolutionary progress. Any other possible mode of accounting for the evidence these lands lie near his north-south belts of greatest oscillatory climatic change and offset the arguments drawn from Equador and Sumatra. The early Tertiary fauna preserved in Madagascar needs especially some explanation since at that time this region according to Reibisch would have been near the south pole. However such objections can always be met and conquered by a sufficiently ingenious advocate.

All hypotheses of polar migration require that there should be enormous changes of figure of the earth in order that the surface for every position of the axis should be in approximate equilibrium. These changes in the earth's body are supposed to take place isostatically, with only a moderate lag. There would be involved however a considerable stretching of those parts of the crust advancing toward the equator because of the greater equatorial circumference, compression in those parts approaching the poles. Several advocates have tried to read into the known crust movements an agreement with these requirements. But as many conflicts as agreements

could be cited, and it is not evident how sharply differential movements like the raising of the east African plateaus and the sinking of the Red Sea and Mediterranean basins can in any respect be responses to such a general change of figure.

The causes of the existence of Permian glaciation in low latitudes constitute one of the unsolved problems of geology, and the phenomena have been utilized by the various creators of hypotheses, but each hypothesis raises difficulties as great as those it is invoked to explain. Although the pole as located in the Permian by Kreichgauer would bring South Africa into the Antarctic circle, the Permian glaciation of Brazil and Australia would still be within the torrid zone. The Permo-Carboniferous axis as located by Jacobitti, while giving antipodal polar latitudes to northern South America and to Australia, would throw glacial South Africa into the torrid zone. The pendulation hypothesis of Reibisch, while permitting polar latitudes to invade Africa, would never give high latitudes to either India or South America.

The advocates of polar wandering have come near to agreement upon one supposition,—that in the Pleistocene the pole was in Greenland, or to the east of it, giving higher latitudes at that time to the glaciated regions of northeastern North America and northwestern Europe. This would imply a polar movement of as much as 15° since the latter part of the Pleistocene. Reibisch in his first papers cites the fact that during the glacial period the volcanoes of equatorial Africa were glaciated to elevations 800 to 1,000 meters below the present limits. He regards this as a proof of his pendulation theory on the meridian $E. 10^\circ$, Africa then having a more northerly latitude. According to this, however, there should just as definitely have been no climatic change in the equatorial Andes, since these are adjacent to the oscillation pole. The fact that in Peru glaciation descended to altitudes below the present limits comparable to the descent on the equatorial mountains of Africa is, however, a most embarrassing fact not cited by Reibisch. For those who would move the Pleistocene pole

into Greenland, these facts of glacial advance in Peru beyond the present limits are even more disconcerting, since their position of the pole would bring Peru directly under the equator during the Pleistocene. Simroth, who goes far beyond Reibisch in his detailed discussion, does note and explain away these difficulties. He states (p. 533) that Reibisch had in a third, still unpublished work reached the important conclusion that a more northerly position of the Alps of only 3° or $3\frac{1}{2}^\circ$ was necessary. The resulting elevation above sea level would be sufficient to originate the glaciation. In regard to the glaciation of the tropical mountains which according to others indicate a general lowering of terrestrial temperatures during the Pleistocene, Simroth says (p. 531):

Here it becomes our duty to go at least a little into argumentation. Kilimandjaro presents no difficulty. It lies so near the oscillation circle that pendulation could have easily carried it into other and cooler latitudes. During our Diluvium it must have lain well under the equator or somewhat north of it, but certainly not near either the north or south pole. One must, therefore, refer it back to the Tertiary in order that it should be permitted to wander to the south pole. There comes then the first thought in regard to those moraines; we do not know their age.

The problem as to how these moraines could have been preserved from erosion since the middle Tertiary is not entered upon by the author. Space forbids further quotation, but Simroth suggests as another alternative explanation that the glaciers may only appear to be far above their terminal moraines because visited in the dry season, during which a rapid melting takes place. As the moraines in question are stated elsewhere to be at elevations 800 to 1,000 meters below the present fronts of the glaciers, this would be a rapid seasonal melting indeed. His elimination of the difficulties connected with glaciation in the Andes is of a similar character.

In view of these quotations from Simroth it should be said that the great part of his work consists of a presentation of biological evidence. In this he is at home and his maps and text bring out many significant facts re-

garding the distribution of animals although some errors could be pointed out. The biologic evidence can, however, all be interpreted by other hypotheses than that of a polar pendulation.

The previous discussion has been given to show the vague and warped evidence upon which a system of terrestrial mechanics has been raised. But this is really not the way to test the hypotheses. They must stand or fall by the astronomical and mathematical implications. What then is the astronomic evidence?

Euler long since pointed out that in a rigid spheroid, if the axis of rotation did not exactly coincide with the axis of figure, the former would revolve around the latter. For the earth, if absolutely rigid, this revolution of the pole would be completed in 305 days. In 1890 Chandler showed that there was such a motion, but that the period was about 428 days. In 1892 Newcomb showed that the discrepancy between the calculated and the observed period was owing to the fact that the earth was not absolutely rigid. The difference in the period implied an elasticity of the earth's body comparable to steel, but did not show plasticity. The motions are confined within a circle about fifty feet in diameter. The actual path is not, however, a circle, and Chandler later showed that it was composed of two harmonic terms, the one about 430 days, the other 365 days. The former is the motion previously described, and is called the Eulerian nutation; the latter is regarded as due to seasonal changes in precipitation and in the seasonal shifting of atmospheric and oceanic currents. There is no suggestion of a third component of polar motion represented by a progressive shifting in one direction. Such a motion even if a fraction of a foot per year would have become evident owing to the length of the time over which refined latitude observations have been made. How does this observed fixity of the axis compare with the demands of the hypotheses of polar migration?

A movement of as much as 10° since the late Pleistocene, would apparently be at a much faster rate than the previous migra-

tions. Overlooking, however, this anomaly of changing rate, suppose the time to be as long as 200,000 years. This great length of time would minimize the annual rate, giving a movement of 18 feet per year. If the movement of the pole is reduced to 3° , as suggested by Reibisch in a later work mentioned by Simroth, this would be at an annual rate of 5.5 feet per year. The absence of even a small fraction of this motion within the period of precise astronomic observations would require the added supposition that progressive migration for some unknown reason had greatly slowed down or that pendulation was at its turning point. The astronomic evidence lends, therefore, no support whatever to the doctrine of a wandering pole.

Apparently Simroth thinks that the movement of precession involves a motion of the earth's axis within its body in a circle of more than 20° radius (pp. 534-536). This, according to him, is combined with the pendulation movement, the result being that the path of the pole is like the projected thread of a screw of which the axis is the meridian 10° E. In following out this idea under the title of the "Probable True Path of Pendulation" he naïvely says:

Possibly there speaks already in favor of a motion of the north pole in a screw line instead of a circle the uncertain statements of the handbooks. One reads now of 25,000, now of 28,000 years. I am not able to judge whence the different figures come. Do they not lie perhaps in the insecurity of the calculated elements which have been considered as circular arcs while they are in truth part of a screw line?

The final test of polar migration lies, however, in the mathematical analysis of the terrestrial motions. Mathematical astronomers have in general been opposed to the idea of a changing axis of rotation, the permanent fixity of the axis having been asserted by Laplace and many others since his day. This problem has been investigated further by Lord Kelvin, but, as previously stated, more especially by G. H. Darwin. The work of these men has been cited as offering no objection to a large or even indefinite wandering of the

pole. An examination of their original papers shows, however, that although they concede limited wandering to have possibly taken place under certain conditions, yet these conditions can not be admitted as existing throughout geologic time. Darwin's paper² is most thorough and conclusive. In it he shows that the axis of rotation will follow the axis of figure. That is, if profound subsidence of miles should take place at some locality, say Boston, and also at its antipodal point, until the connecting line was the shortest diameter of the earth and if there should simultaneously occur an upheaval around a great circle at ninety degrees from this point until this circle should constitute an equatorial bulge, then, and only then, could Boston come to lie on the axis of the earth.

If the change took place cataclysmically and the earth were sufficiently rigid, there would be set up a permanent Eulerian nutation, or circular wobbling of the pole, but if the change was slow and intermittent the Eulerian nutation would never be large. The lack of cumulative effect would be due to the variable positions of the instantaneous axis of rotation with respect to the principal axis of the earth at the times of successive impulses.³ Thus, as a result of movements through the earth's body, shiftings of the axis of rotation would take place, keeping it close to the axis of figure. The axis of rotation at any time is consequently stable. To change it there must be shiftings of matter in order to change the axis of figure. As the radii would have to change in length by many miles for an extensive migration, the mere gradational processes of erosion and sedimentation could not be of much effect. There would have to be internal changes of form far greater than the known amounts of uplift and depression. Any explanation as to what force could cause the

earth to expand in one direction and contract in another direction to these great amounts is absent. Apparently the earth would have to be granted an amœboid power, which Simroth as the sponsor of the pendulation theory and a biologist might be willing to confer. By assuming a plastic earth and convective movements in its internal mass, energy could be supplied and a considerable polar wandering result, the process being analogous to a protoplasmic streaming. Lord Kelvin granted the possibility of a considerable polar wandering during the early plastic stage of the earth, but held that practical rigidity had prevailed throughout geologic history.⁴ These statements are sufficient to show the conflict between the mechanics of a revolving solid globe and any hypothesis of unlimited wandering through geological time.

But movements of elevation have gone forward in some places, of subsidence in others. What maximum polar shifting could be the result of such continental and oceanic movements? Darwin has given a quantitative solution to this question. Taking the areas as in the most favorable situation to affect the axis of rotation, he assumes that one area is elevated 10,000 feet and another equal area subsides 10,000 feet. A table shows the relation between the size of these areas and the resulting deflection of the pole. A land mass as large as Africa thus favorably situated and undergoing reciprocal vertical movement with a section of oceanic bottom of like area, would result in a deflection of the pole amounting to about two degrees. If such changes were *progressive* and in the right direction Darwin states that they might account for a change of 10° to 15° since the consolidation of the earth. The kind of progressive changes which would account for this amount of shifting have not, however, been shown to have occurred through geologic history.

To affect the position of the pole the most favorable situation is for uplift to occur at two antipodal regions in latitude 45°; for depression to take place on the same meridian circle,

⁴ *Trans. Geol. Soc. Glasgow*, Vol. XIV., p. 312, 1874.

² "On the Influence of Geological Changes on the Earth's Axis of Rotation," *Phil. Trans. Royal Soc.*, Part I., Vol. 167, 1877, pp. 271-312; Vol. III., Collected Works.

³ G. H. Darwin, "On Professor Haughton's Estimate of Geological Time," *Proc. Royal Soc.*, XXVII., pp. 179-183, 1878.

but in the opposite quadrants from the uplifts. This amounts to a shifting of matter from two antipodal regions to regions 90° from them, but on the same meridian circle.

Erosion and sedimentation serve only to transfer sediment from the high parts of a continent to its low interior or its borders. The limestones may be partly deposited in other regions of the earth, but they constitute not over ten per cent. of the sediments. Isostatic readjustments would tend to affect the regions unbalanced by erosion and sedimentation. All of these actions have had but little tendency to shift matter from one octant of the earth's surface to another octant. Such surface processes have consequently had but little effect in shifting the poles.

The greater factor lies in the fragmentation of ancient continents, assuming that the possibility of this process be granted. But much of the Pacific must always have been a reservoir for the ocean waters. The fragmentation of Laurentia, extending the North Atlantic ocean basin, would largely be balanced against the sinking of Gondwana to form the South Atlantic. Downsinkings in the Indian Ocean and in the tropical Pacific would have but little effect since they lie mostly within the torrid zone. These down-sinkings, furthermore, need not have caused to bulge up by just that much some particular continent or continents. The up-swelling to compensate for the down-sinking may more readily be conceived as affecting the whole earth. Fragmentation, therefore, has not been areally distributed in such a manner as to produce the maximum effects calculated by Darwin as possible from vertical changes of 10,000 feet.

There is still another vital consideration, however. Darwin considers the case where elevation and subsidence is due to change of density, but not change of mass. Taking a superficial layer ten miles thick as not changing, but a swelling to occur throughout a section of crust from ten to fifty miles in depth, the change in the position of the axis would be but .0126 of what it would be if the uplift were due to an addition of matter. The pertinency of this is seen if it be noted that the

great plateau uplifts of the Tibetan region in Asia, of the Cordillera of North and South America, have been upraised with an approach to isostatic equilibrium from a state of low elevation and broad submergence in the early Tertiary. This is quite commonly viewed as the result of an intumescence in the crust beneath, due perhaps to the irruption of magmas and their accompanying heat and to the heat of orogenic deformation. But Darwin's figures show that uplifts due to this cause have a negligible effect upon the axis of rotation. Continental fragmentation and the sinking of Mediterranean basins, to such extent as they may have gone forward, may have been due to some contrary process of increasing density, the regional vertical movements thus conserving the isostatic principle.

From these considerations it is seen that closer examination tends to cut down more and more even those moderate limits of polar migration set by Darwin. It would appear that the assumption of polar wandering as a cause of climatic change and organic migrations is as gratuitous as an assumption of a changing earth orbit in defiance of the laws of celestial mechanics. Unless some wholly unsuspected forces are at work within the centrosphere, polar wandering has no more basis in science than Symmes' imaginings of a hollow earth. From all that is known at present the doctrine must be regarded as a vagrant speculation, not as a working hypothesis.

In closing this article it seems appropriate to indulge in a brief moralization. This paper does not contribute any new facts, but was written to show the untenableness of certain hypotheses, emanating in this instance from Germany and in danger of spreading in America, by confronting them with observed facts and mathematical demonstrations, much of which, originating in England and America, has been in the possession of science for more than a quarter of a century. Does not the history of this subject show the dangers of over-specialization within one division of science with the consequent putting forth of hypotheses regardless of the verdict of related

sciences? It certainly shows admirably the defects of the advocating method of research—the dangers of the ruling hypothesis. Probably also a more respectful reception has been given in this country to these hypotheses because they were voluminously presented in German and backed by the prestige of a German professorship, than if they had originated in this country. But if the writer is not mistaken, in Germany, preeminently the land of science, voluminous presentation is a fashion, and around the large body of high-grade work is a larger aureole of pseudo-science than is found in either England or America. We are sadly in need of knowing more German and in making larger use of foreign literature, but discrimination is necessary, and the writer is inclined to think that some Germans in turn might make larger use of scientific literature in the English language.

JOSEPH BARRELL

SCIENTIFIC NOTES AND NEWS

THE New Zealand meeting of the British Association has been abandoned. It will be remembered that a number of distinguished American men of science are on the way to attend the meeting as guests of the New Zealand government.

SIR ADOLPH ROUTHIER has been elected president of the Royal Society of Canada in succession to Professor Frank D. Adams.

THE commission authorized by the New York state legislature to undertake the scientific study of the causes of bovine tuberculosis, its economic and health effects upon the state, has been appointed by Governor Glynn. The members of the commission include: Dr. Th. Smith, director of the division of animal pathology, Rockefeller Institute; Dr. Hermann M. Biggs, commissioner of health, New York; Dr. Linsly P. Williams, deputy commissioner of health, New York; Dr. Philip Van Ingen, of the New York Milk Commission; Dr. Henry L. K. Shaw, professor of children's diseases, Albany Medical College; Seth Low, and Professor Veranus A. Moore, dean of the New York State Veterinary College, Cornell University.

THE Paris Academy of Sciences has awarded its La Caze prize of \$2,000 to Dr. Gley, professor at the Collège de France, for his works on physiology.

THE Sir Gilbert Blane medal of the Royal College of Surgeons of England has been awarded to Surgeon G. F. Syms, R.N.

DR. ALEXIS CARREL, of the Rockefeller Institute for Medical Research, has been made director of the Military Hospital at Lyons, throughout the war.

It is said that Dr. A. L. Skoog, professor of neurology in the University of Kansas, has been made temporary head of the La Petrie Hospital in Paris. Dr. Skoog was doing clinical work at the institution when the entire hospital staff was obliged to undertake military service.

DR. AUGUST LYDTIN, the author of important contributions to veterinary medicine and animal breeding in Germany, has celebrated his eightieth birthday.

THE first of the short addresses at the dedication of the new building of the Marine Biological Laboratory published in *SCIENCE* for August 14, should have been attributed to Professor Frank R. Lillie, director of the laboratory.

MR. C. A. McLENDON, botanist and plant pathologist of the Georgia Experiment Station, has accepted a position with the South Carolina Experiment Station as field pathologist. Mr. McLendon succeeds Mr. L. O. Watson who has gone to the Bureau of Plant Industry to take charge of the cotton wilt work in the south.

PROFESSOR CHARLES P. BERKEY, of the department of geology, Columbia University, accompanied by Dr. Clarence N. Fenner, of the Geophysical Laboratory, Washington, sailed from New York on August 15 for Porto Rico to make a geological reconnaissance of the island. This party represents the New York Academy of Sciences, which has undertaken, in connection with the government of Porto Rico, a complete natural history survey of the island. It is hoped during the present season